Review of Biogeosciences manuscript bg-2021-71,
"Towards Estimation of Seasonal Water Dynamics of Winter Wheat from Ground-Based L-Band Radiometry"

by Thomas Jagdhuber, François Jonard, Anke Fluhrer, David Chaparro, Martin J. Baur, Thomas Meyer and María Piles

Summary:

The manuscript presents a radio-meter based approach along with on-site measurements to estimate seasonal flux rates of water over a winter wheat field. The paper is well written, and the manuscript exhibits useful results. There are just a few aspects that need to be addressed before publication. First, the paper lacks other sources of data (e.g., satellite products and/or field laboratory data) to validate the employed empirical models and results. I'd suggest the authors at least include a few other observations to validate the overall utilized approach. Second, the paper requires some further modifications and/or clarifications in different parts. Based on these shortcomings, I recommend a minor revision. The authors should consider the following comments in their revision.

Major Comments:

Comment (1): Line 105: Why this particular plant has been selected for this study? What are the characteristics that distinguish it from other plants? How does selecting a plant and its hydraulic traits influence the final conclusions of the research? The authors need to comment on these.

Comment (2): Equation 6: This model seems to have some empirical coefficients. Are these coefficients plant-type dependent? In Lynn and Carlson (1990), Fig. 16 is depicted for corn. How can that impact the used model in this study? The authors need to comment on these.

Comment (3): Figure 11 and Line 420: Something that perplexes me is that the LAI is changing nonlinearly in the whole duration of the measurements according to Figure 6 implying that the total biomass is changing. If this is true, the comparison shown in this study does not seem valid (based on Line 420) and does not add anything to the paper.

Comment (4): Figure 11, and a general comment: In general, one downside of the paper is that it does not compare the obtained results with other remote sensing products and/or laboratory analysis. This is significant for validation of the employed empirical models and results. In particular, authors can compare their derived RWC_{VOD} (Fig. 11) or soil moisture with satellite

products. Although the resolution might be different, it is expected to see generally a similar trend that can further validate the employed methods.

Comment (5): Line 560: How can such water flow estimations be done solely using remote sensing data? The authors could add some discussions on this and the deficiencies of remote sensing approaches to fully capture the water flow dynamics. I noticed that this has been somewhat discussed in lines 610-620 but more discussions focusing on the limitations and deficiencies of such remote sensing data would be useful especially for large-scale studies.

Minor Comments:

Comment (1): Line 125: How far is the climate station from the measurement site?

Comment (2): Figure 1: How much is VWC correlated with LAI?